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JUN 21 1913

BACTERIOLOGY

A REVIEW AND AN OUTLOOK

An Inaugural Lecture

delivered before the University of Liverpool

on Friday, December 6, 1912

BY

J. M. BEATTIE, M.A., M.D.

PROFESSOR OF BACTERIOLOGY

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57 ASHTON STREET. 1913



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BACTERIOLOGY:

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THE President of the British Association, in his address in September of this year, said, 'It is not too much to assert that the knowledge of the parasitic origin of so many diseases, and of the chemical agents which on the one hand cause, and on the other combat, their symptoms, has transformed medicine from a mere art practised empirically into a real science based upon experiment. The transformation has opened out an illimitable vista of possibilities in the direction not only of cure, but, more important still, of prevention.'

These words call forth a most hearty response from all who have knowledge of the facts, and it is not unnatural that the bacteriologist who, with the chemical physiologist and pathologist, has played so important a part in this great work should feel dissatisfied till his science receives its proper recognition in the science curriculum of our Universities.

This great transformation has taken place during the lifetime even of those who are still ardent workers in the field. When I started the study of medicine, bacteriology had a very small place in medical teaching, and the class was attended by about half a dozen enthusiasts. Even to-day it does not form a subject of study in the medical curriculum of some of the great medical schools of this country. I do not state these facts by way of complaint or of criticism, but merely to shew that the importance of the subject is not yet fully recognised even in those circles where its triumphs are so obvious.

And yet, bacteriology is not by any means a new subject, for between 1675 and 1683 Anthony von Leeuwenhock, by virtue of his perfectly-constructed lenses, first beheld bacteria and called them 'animalcula.' Not only did he see the organisms, but he recognised differences in their appearance and in their size, and, further, that they were present in various substances of animal origin. This discovery naturally gave a new interest to discussions which had taken place as to the relationship of disease to a living virus. With great epidemics of disease which devastated towns and cities and whole kingdoms, it would have been strange if there had been an absence of investigators—if everyone had obediently followed the dictum that it was the will of the gods, and, therefore, did not concern man. Fortunately, in every age, there has been at least one Thomas, and this man, or these men, found that disease spread from individual to individual, and that the isolation of the diseased person was a precaution necessary to stop the spread of an epidemic. Consequently we find in Biblical times, and even in old Babylonia, the establishment of isolation camps, and there are running through the writings suggestions at least that the outbreaks of disease were due to some living agent which could pass from person to person.

About 1260, Theodoric in Bologna taught the first principles of antiseptic surgery. He pointed out that the action of the air on wounds predisposed to suppuration, and in order to lessen this risk he bathed his wounds with hot wine. Later, in 1306, Henri de Mondeville got even nearer to the present-day surgeon, for he emphasised the necessity of closely approximating the edges of a wound, and then, to prevent risk of infection from

the air, he covered it with a plaster composed of turpentine, resin and wax, and then applied the hot wine fomentations of Theodoric. It was not, however, till 1671 that Athanasius Kircher, the real discoverer of the microscope, boldly declared that puerperal fever, purpura, measles and various other fevers were caused by putrefactive changes brought about by worms or animalcules. This opinion was received, as new opinions are even to-day received, with the greatest scepticism. His opinions were founded on very feeble evidence, but they reflect the spirit of the scientific workers of that day. These men had been groping in the darkness for years, but the light was beginning to dawn. Only a few years later came the announcement of the seeing of these living organisms by Leeuwenhock and our first actual introduction to the science of Bacteriology.

Then, in 1762, Plenciz, a Vienna physician, declared not only that disease in animals and in man was caused by these 'animalcula,' but that each disease had its specific organism. We need not wonder that such advanced views were scorned by the contemporaries of Plenciz, and that they were soon forgotten. It was not till 1837, a year ever memorable in the history of this nation, as well as in the science of bacteriology, that another step was taken in the ever onward march both of civilisation and of science. In that year Latour and Schwann, working independently, shewed that the small round bodies, first found by Leeuwenhock in beer and in wine, could be seen to increase and multiply as fermentation proceeded; and they were the first to suggest that these were minute plants and the cause of the fermentation. In the same year Bassi proved that muscardine (the

contagious disease of the silkworm) was caused by a minute plant, a specific germ. This discovery, confirming as it did the views of Latour and Schwann, gave a great impetus to the study of bacteriology, and hosts of workers were soon engaged in this promising field of research. All sorts of microscopic forms were now described in disease conditions, and their causal relationship to these diseases accepted without question. The result was confusion and a bar to future progress. The question, however, was placed on a more definite basis by Jacob Henle, of the University of Berlin. Examining all the facts which had been brought forward by various observers, he concluded that infectious diseases are caused by living organisms, but that in order to establish this for any specific disease the organism must be found *constantly* in the infectious material, and must be isolated from it and its strength tested. Henle must be regarded as the real founder of the 'Germ Theory of Disease.' Henle's views were not accepted, but the rigid proof demanded by him made a profound impression on the thought of his time.

His work had the effect of checking the wild and reckless speculation which had become a serious menace to further real progress. Fortunately, good work went on, and in the next decade a host of parasites were described, many of them, as we now know, belonging to the higher fungi. The years 1850 to 1860 shewed great advances. Pollender and Davaine succeeded in demonstrating the presence of the anthrax bacillus in the blood of animals suffering from 'Splenic Fever,' and Davaine in 1863 demonstrated by experimental methods that the bacillus was the actual cause of the disease. This, I believe, was the first bacillus proved

to be specific for a disease. About this period, that great observer, Pasteur, first enters the bacteriological world, and in 1857 his demonstration of the special resistance to heat of certain bacterial structures, which we now call 'spores,' dispelled the fascinating doctrine of spontaneous generation. Even 600 years before Christ, among the early Greeks the theory that animals were formed from moisture was held, and Aristotle asserts that 'sometimes animals are formed in putrefying soil, sometimes in plants and sometimes in the fluid of other animals.' Van Helmont gives special instructions for the artificial production of mice, and Kircher, in his 'Mundus Subterraneus,' describes and actually figures certain animals which were produced under his own eyes by the transforming influence of water on fragments of stems from different plants. The observations of Leeuwenhock gave a new impetus to the work on spontaneous generation, for it seemed an easy transition from minute inanimate particles to the primitive forms of life which he had described. The battle raged fiercely round this subject, and its clouds obscured much valuable work which, if proper attention had been given it, might have led us to an earlier appreciation of the work of Plenciz, Schwann, Henle and others. In spite of the great work of Pasteur and of the previous work of Spallanzani and Schulze, the theory of spontaneous generation still found its supporters, and in 1875, at a meeting of the Pathological Society of London, Dr. Bastian opened a debate in which he strongly supported the view that the co-existence of bacteria and contagious disease was merely evidence that the bacteria were pathological products spontaneously generated in the body after it had been rendered diseased by the real contagion. Dr. Bastian

approached the subject, not as a theorist, but as an experimentalist, and he was quite prepared to submit his views to the test of rigid experiment. He maintained and, I may add, still maintains that if fluid extracts were boiled and the vessel sealed to prevent access of air, in a very short time the sealed flasks shewed many bacteria and allied organisms. All bacteriologists are agreed that Bastian's case has been abundantly disproved, and I would not have referred to the subject to-day were it not that it has been brought prominently to the front in the address from which I have already quoted, and that Bastian only last week put forward fresh evidence in favour of this view. Schafer, after pointing out that the elements composing living substance are few in number, carbon, hydrogen, oxygen, nitrogen and phosphorus, with certain inorganic salts, magnesium, potassium and iron, says, 'The combination of these elements into a colloidal compound represents the chemical basis of life; and when the chemist succeeds in building up this compound it will, without doubt, be found to exhibit the phenomena which we are in the habit of associating with the term life.' He is careful to guard himself against being considered a disciple of Bastian, though some of those who, for their own purposes, have misrepresented his views, have not been so careful, for he says, 'We cannot deny or even doubt the evidence of almost every worker in this realm of science that properly sterilised fluid remains sterile so long as the germs present in the atmosphere are properly excluded. Besides, both the structural and functional differentiation of these minute organisms is not what we would expect in these newly-created beings. Our mistrust of such evidence as has been brought forward,' he adds,

‘need not, however, preclude us from admitting the possibility of the formation of living from non-living substance.’ From this it will be seen that the doctrine propounded by the President of the British Association is not the old one of spontaneous generation, but a transference of non-living to living matter by a process of gradual evolution. It must be admitted that very little evidence has been brought forward to support even this modified doctrine. It is true that we have diseases produced by viruses which cannot be detected even with the aid of the most powerful microscopes, and which have also eluded the methods of the chemist. These ultra microscopic bodies may be regarded, as they are by Bastian, as the progenitors of the minute microscopic organisms with which we are familiar, but the objection which forces itself on the bacteriologist is that in all his experience he has not been able to detect any transition forms which lead up to the minute organisms which have been known for so long, and one would expect this, if a *gradual* evolution rather than a sudden transformation from non-life to life is to be admitted. It may be that these transition forms have not been looked for, or, having been looked for, have not been recognised. Before the searchers in the world of the minute, there may be the solution of the great problem—the origin of life. As Schafer has well said, ‘When or where the change from non-living to living matter may first have occurred, when or where it may have continued, when or where it may still be occurring, are problems as difficult as they are interesting, but we have no right to assume that they are insoluble.’ Well might we say to the student of bacteriology, the field before you is illimitable.

Fortunately, during the battle on spontaneous generation there were many workers who were not drawn into the fight, and who were able to carry on the work inaugurated by Bassi, Henle, and Schwann. Pasteur's observations on fermentation, which have had such an important bearing on the whole subject of bacteriology, were carried on at this time. He shewed that the fermentation which produced lactic, acetic and butyric acid was brought about by micro-organisms which differed from one another in morphological and biological characteristics. Thus he established on a firm basis, not merely that there were distinct micro-organisms differing in morphological characters, but that these different organisms could produce different physiological effects. This view received confirmation from the chemical side by Lemaire, who shewed that the fermentation could be arrested by the addition of small quantities of carbolic acid, and that the addition did not destroy the action of diastase and other fermenting bodies. It was soon shewn that the carbolic acid had the effect of inhibiting, and even destroying, the living organisms which were the producers of the ferments. These observations at once opened up a great field for investigation, and also for practical work. It had been established that the non-healing of wounds, and particularly the suppuration of them, was due to the presence of these minute living bodies, and Lemaire at once suggested the use of carbolic acid as a wash for the wounds. It was shortly after this that Lister began those epoch-making researches which afterwards placed his name at the head of the rôle of the benefactors of mankind. He was apparently unacquainted with the work of Lemaire, and seems to have got his first idea of the efficacy of carbolic

acid as a germ destroyer from the work which was being done on the purification of sewage in Carlisle.

His idea, and the idea of the workers of that day, was that the germs gained access to the wounds from the air, from the hands of the surgeon, from the instruments, etc., and that, therefore, it was necessary to destroy these germs with carbolic acid. Hence we had the surgeon almost bathed in carbolic, and his whole work carried on most uncomfortably in a spray of carbolic. How different things are to-day ! The old methods were based on insufficient knowledge ; but let us not forget the steps by which we have climbed to the heights of perfection, and, least of all, let us forget that the first step was laid by that illustrious son of France, Pasteur, and that the second was built as firmly by an equally illustrious son of our own land, Joseph Lister. It is interesting to recall the words of Lister and those of another of the great workers in the field, Tyndall, which were addressed to Pasteur at the celebration of his jubilee in 1892. Lister said, ' Truly there does not exist in the entire world any individual to whom the medical sciences owe more than they do to you. Your researches on fermentation have thrown a powerful beam, which has lightened the baleful darkness of surgery, and has transformed the treatment of wounds from a matter of uncertain and too often disastrous empiricism into a scientific art of sure beneficence. Thanks to you, surgery has undergone a complete revolution, which has deprived it of its terrors and has extended, almost without limit, its efficacious power ' ; and Tyndall says, ' We have been scourged by invisible thongs, attacked from impenetrable ambuscades, and it is only to-day that the light of science is being let in upon the murderous dominion of our foes.'

During the ten years 1866 to 1876 great advances were made, but advances, however valuable and however far reaching, often lead to confusion and to doubt, because the one essential point for the proper appreciation of the results has been lost sight of or has not been discovered at all. Thus it was that these ten years of research rendered the darkness almost more intense, and, at the end, it seemed also impenetrable. True, there were a few observers who were convinced that there was one path through this gloom, but in spite of many attempts they were unable to discover it. Chief of these was Hallier, who, at the beginning of this period, realised that little real advance could be made until some method was discovered for the separation of micro-organisms from mixtures. The apparatus which he devised, however, was not satisfactory, and his results only led him further into the darkness. His explanation of the various shapes and sizes which his micro-organisms shewed was that they were pleomorphic and could develop one from the other. Wrong as his conclusions were, his work was extremely valuable, not only because it led other workers to seek the path which he had failed to find, but because during his work he was able to shew that there were present in glanders, diphtheria, gonorrhoea, measles and other diseases bacteria which were probably causal. Numerous other observers, about this time, demonstrated what they believed to be bacteria in many morbid processes. There seems little doubt that these bodies were in many cases cellular detritus of organic and inorganic nature, particles of fat and various crystals, but the observations of von Recklinghausen in 1871, that in the tissues of individuals dead of pyaemia, of puerperal fever, of typhoid fever, or of

what to me is of special interest, rheumatic fever, there were bodies which were not dissolved by alkalis, by acids, or by glycerine, seem definitely to point to his demonstration of bodies, to say the least, of organic nature and probably bacterial. In the same year there is no doubt that Waldeyer clearly shewed the presence of groups of bacteria of coccal nature in the heart muscle from cases of pyaemia. About the same time, Weigert drew attention to the presence of bodies corresponding chemically to those described by von Recklinghausen in the skin in cases of haemorrhagic smallpox.

Then, in 1871, Klebs, in his investigation during the Franco-German war, traced the entrance and the development of bacteria in wounds and their passing into the circulatory system, and he described his '*microsporion septicum*,' which he found in gunshot wounds, in the cartilages of joints and in the bone marrow in cases of diphtheria. Like Hallier, Klebs attempted to separate the organisms from one another and to obtain pure cultures, but all his attempts resulted in failure, and he concluded that such diverse pathological conditions as puerperal fever, phthisis, smallpox, diphtheria and cholera were caused by the same micro-organism. In this view he was strongly supported by Bilothe. Thus at the end of this period of ten years Weigert, Cohn, Eberth, Rindfleisch, von Recklinghausen, Waldeyer, Klebs and others had shewn that bacteria were constantly present in diseased tissues while they were not able to demonstrate them in normal tissues, and though they differed among themselves as to the sources from which these organisms came, they were agreed that they must be regarded as the causal agents of the pathological changes. But in spite of this valuable work there was still no definite proof, and

the path which Hallier and Klebs had sought in vain still remained to be found. But this path was not long to elude the diligent seeker, and in the Botanical Institute of Breslau, where Ferdinand Cohn had been doing patient, valuable work, Schroeder cultivated on various media the '*monas prodigiosus*' and a number of other pigmented bacteria. Though to Schroeder and his master Cohn must be given the first place in this important advance, it is only just to state that the '*monas prodigiosus*' was discovered by Ehrenberg, and actually cultivated on potato by Fresenius and by Erdmann. Neither these observers, nor Schroeder himself, were able to be certain that their cultures were pure, and that associated with the colour-producing bacteria there might not be others of a similar morphological character—a very important matter, if the question of specificity for separate diseases was to be established. Cohn fully realised that little progress could be made in this direction until the organisms were grown in pure culture and watched over long periods of time, and he regarded the discovery of some method by which this separation of one organism from another could be with certainty carried out as the key to the main problem which was then facing the bacteriologist. On April 22, 1876, Cohn received a letter from a young country physician in Wollstein, stating that he had worked out the life-history of the anthrax bacillus, and that he was anxious to demonstrate his work to Cohn and to receive his judgment on it. Cohn eagerly invited the young unknown man to his laboratory, and on April 30, Robert Koch began a three days' demonstration in the Botanical Institute in Breslau. It is unnecessary to detail his experiments; suffice it to say that he

proved to the satisfaction of Cohn that his claim to have worked out the complete life-history of this bacillus had been established. He cultivated the organism on artificial media, he shewed it producing spores, and from the spores he shewed the growth of the new elements. With his spores and with his fully-developed organism he inoculated various animals, and in them produced typical anthrax, and finally he proved that the nature and the distribution of the disease could be explained by the facts which he had established in regard to the life-history of the causal bacillus. This demonstration, memorable in the history of bacteriology, was witnessed by Leopold Auerbach, Julius Cohnheim and Karl Weigert, and it is related that Cohnheim was so delighted by what he saw that he rushed back to his Pathological Institute and urged all his assistants to hurry to the Botanical Institute and witness the startling discovery. 'It leaves nothing more to be proved,' said Cohnheim, 'I regard it as the greatest discovery that has ever been made with bacteria, and I believe that this is not the last time that this young Robert Koch will surprise and shame us by the brilliancy of his investigations.' Koch's results were shortly afterwards published, and this paper is one of the great classics in bacteriology. The development of bacteriology has not thrown doubt upon the accuracy of Koch's observations, nor disproved any of the conclusions which he drew.

There was some opposition to the work of Koch in France, and this led Pasteur into the field of infectious diseases. He confirmed all of Koch's observations and overthrew all opposition. Progress now became very rapid, and especially in Koch's laboratory in Berlin, where, through the influence of Cohnheim, he had been asked to accept

the post in the Gesundheitsamt which had been vacated through the acceptance of a professorship by Finkelnberg. From his laboratory contributions of the greatest importance in bacteriology were published. Time will not permit me to do more than summarise the work of the years 1876 to 1890.

Ford has well said that 'in the decade from 1872, when Koch returned from the Franco-Prussian war, to the year 1882, the science of bacteriology had changed from a chaos of conflicting views and observations to a well-ordered system. The methods of fixing and staining bacteria so as to make their microscopic study possible had been devised, their cilia had been stained, the oil immersion lens and the Abbe condenser had been utilised, a method of separating single species from mixtures had been found, the pathogenetic bacteria had been cultivated in pure culture, the etiology of infections had been cleared up, the germ theory of disease had become a proven doctrine.' For most of this advance in knowledge Koch was himself responsible. Now that the path through the darkness had been actually trodden, it is not surprising that discovery followed quickly on discovery. In 1879, Hansen announced the discovery of the bacillus of leprosy, and Neisser discovered the gonococcus. In 1880, the bacillus of typhoid fever was observed by Eberth and by Koch, Pasteur published his work on chicken cholera and Sternberg described the pneumococcus.

The year 1882 is even more memorable, for in it Koch worked out the etiology of tuberculosis by his discovery of the tubercle bacillus. To do this he devised a special method of staining, and finally he succeeded in growing the organism in pure culture on solidified blood serum and proving its relationship to the disease by inoculation experi-

ments. The same year saw the discovery of the bacillus of glanders by Löffler.

In 1883, Koch went to Egypt, and then to India, as the leader of the German Cholera Commission, and in 1884, his painstaking investigations were rewarded by the isolation of the Cholera vibrio and his establishment of its etiological relationship to the disease. Of special importance was his cultivation of the organism from drinking water and from articles of food. So important and so valuable was this work that on his return from India Koch was given a donation of 100,000 marks by the State, and Kaiser Wilhelm I bestowed on him the Order of the Crown. This year also brought the discovery of the diphtheria bacillus by Löffler and the organism of tetanus or lockjaw by Nicolaier. In 1890, Koch described the preparation of tuberculin, which was immediately heralded about the world as the great specific for tuberculosis. Whether Koch himself was responsible for this publication or not is a matter of doubt. Many theories have been put forward to explain why these observations, which did not stand the test of the rigid laws which Koch himself laid down, were given to the world. But it would not be profitable to follow these speculations. Suffice it to say that tuberculin as given in 1890, far from being specific, was, in the hands of some, a deadly poison; instead of arresting the disease, it often rendered it more active. Subsequent work has perhaps given us the explanation of this failure, and to-day we can give tuberculin, even the old tuberculin of Koch, not merely without doing harm, but with decidedly beneficial results. This year, memorable for the one failure in the work of the greatest bacteriologist which the world has seen, also opened up a new era in the

history of bacteriology by the marvellous work of Behring in relation to serumtherapy—one of the most triumphant achievements of scientific medicine.

Before referring further to this, let me mention the discovery in 1892, by Pfeiffer, of the bacillus of influenza, and in 1894, by Yersin and Kitasato, of the causal organism of bubonic plague.

The discovery of the causal organism of any disease and the study of its pathology is an essential step in the scientific treatment of disease. As was natural, therefore, during the period in which the causal agents were being discovered, attempts were being made to find some means of destroying the organisms or of combating their effects. Many of these attempts were mere empiricism, and it was only when it became fully established that some of the bacteria produced cell-poisons or toxines, not only in artificial media, but in the animal body, that real progress became possible. It is true that from very remote times protection of the individual against disease was engrossing the attention of humanity, and that some of the native races in Africa had actually discovered means of assuring immunity against snake-bite. There seems no reason to doubt the story that the Portuguese Colonel, Serpa Pinto, tells of his vaccination by some natives of the East Coast of Africa against snake-bite. These savages, we are told, extracted the poison of snakes and prepared from it, by the addition of vegetable substances, a glutinous paste which they introduced under the skin. Pinto was, a short time after he had been vaccinated, bitten by a scorpion, and he experienced no ill effects. Ten years later he was again bitten by a scorpion, and his life despaired of. This seemed to prove that his vaccination had been the actual cause of his first immunity.

The Moors vaccinated their cattle against pleuropneumonia, and the Chinese assert that from the commencement of the 11th century they were acquainted with the method of immunising against smallpox. The Ashantis and the Persians vaccinated regularly for the prevention of smallpox, and it is familiar to everyone that the method of inoculation against smallpox was first introduced into this country from Constantinople by Lady Mary Wortley Montagu in 1721. It is asserted by Haser that in Baluchistan the custom of having cows suffering from smallpox milked by children who had wounds on their hands had been widespread from time immemorial, and we know that Jenner got his first lesson in vaccination from the country people in his native county of Gloucestershire. This discovery of Jenner, which has had such widespread effects, and which we as a nation, in the name of liberty, which so often should be translated licence, are to-day so grossly neglecting, was only the beginning of much experimental work in the field of serum-therapy. Pasteur and his fellow-workers produced vaccines for chicken cholera, for anthrax and for rabies, and a new school, or rather two new schools, sprang into existence. One of these held that the immunity was due to some changes in the body fluids, the other that the process was entirely due to the action of the cells of the body. The former, the humoural school, has, as its great exponent, Paul Ehrlich, and the latter, the equally great investigator, Elias Metchnikoff. Though the work of Metchnikoff on phagocytosis must be regarded as one of the most brilliant achievements in the history of biological science, yet it is largely to the humouralists, Fodor, Nuttall, Behring, and Buchner, that we owe

the great discovery of antitoxins. They discovered that the blood serum of certain animals had a distinct bactericidal action, and, further, that serum of naturally immune animals often had a higher bactericidal effect than the serum of susceptible animals. The next step was the observation made by Behring, that the serum of artificially immunised animals possessed this property in a very marked degree. During the progress of this work it was also noted that in certain cases of acquired immunity no bactericidal substances could be demonstrated in the blood—*e.g.*, in infection with diphtheria bacilli. Explanation was sought, and it was only after Roux and Yersin, in 1888, succeeded in obtaining toxin from a broth culture of the diphtheria bacillus that the possible explanation was put forward. A number of workers in Koch's laboratory attacked the problem of immunity to diphtheria. In 1890, Frankel succeeded in producing immunity by toxin which he had heated to 60° C., and in the same year Behring had similar results. In 1890, too, Behring and Kitasato announced the important discovery that an animal immunised against tetanus or diphtheria produces in its blood, substances which are capable of neutralising the toxic action of the specific poison, that the blood serum, when injected into other animals, can prevent the toxic action, and when injected, even after the onset of symptoms, can save the life of the animal. This substance they called 'antitoxin.' These great results were soon carried beyond the experimental stage by various observers, but it was not till after Roux published his paper at the International Medical Congress at Buda Pesth, in September, 1894, that the antitoxic treatment of diphtheria became established on a firm practical basis, and no one who will fairly face

the facts can fail to be convinced of the enormous saving of human life and human suffering which has resulted from that great work. This question of immunity against disease has already rallied to its solution a host of workers—in fact, we have almost developed in this realm the specialist. It would not be profitable, even if time permitted, for me to lead you into this field. An enormous amount of work has been done—no doubt, much of it has led its workers into blind alleys from which they have had to retrace their steps—much of it has led to confusion and contradiction, and even to despair and scepticism, but when the waste has been cast aside there remains a firm residue on which is being built a great structure, whose true proportions it may still take us years to appreciate. We are all concerned with the *cure* of infective disease, but the advances made in bacteriology are leading us to the day of *prevention*. Of the comparatively recent work of Wright and his school on vaccines, and of the still more recent work of Flexner and his school on cerebro-spinal meningitis, and particularly on infantile paralysis, I need hardly say anything. The importance of this work cannot be over-estimated, and its brilliance ranks with that of the great discoveries of Koch and of Behring.

Ladies and Gentlemen, I have led you very imperfectly through the various stages in the great work which has been done by the bacteriologist, and I now ask what of the future? I do not wish to assume the mantle of the prophet; but he would be blind indeed who fails to see that the onward march, which has become even more rapid as the years have gone by, is not now to cease. No, truly, the goal before us is becoming more and more clear, and the stimulus of the past is only

urging us on to greater efforts to reach that goal—the prevention of infective disease. Bubonic plague, which was once a great scourge of our land, has almost disappeared, though, unfortunately, it still holds sway in our great Indian Empire. Typhus fever is almost unknown, and yet Hirsch tells us that ‘The history of Typhus is written in those dark pages of the world’s story which tell of the grievous visitations of mankind by war, famine and misery of every kind.’ In 1875, there were in England 1,499 deaths from typhus; in 1895, only 58. Typhoid fever is rapidly diminishing. Cerebro-spinal or spotted fever, thanks to the great work of Simon Flexner, has lost much of its terror, and to-day we are rejoicing at what seems to be the conquest of infantile paralysis by that same brilliant worker. And what shall we say of Diphtheria? Yes, Ladies and Gentlemen, I know there are people who tell us that the bacteriologist has done nothing for diphtheria, nay, some even say we have brought a new scourge on humanity. But we need not pause to consider such opinions. The facts are open to all, and no unbiassed investigator can have any doubt in the matter. The death rate, which, before the use of antitoxin, was in the most favourable conditions between 30 and 50 per cent., has fallen to from 12 to 15 per cent., and this could be reduced if we were able to treat the cases in the very earliest stage. Time will not permit me to deal with the great work of Bruce, Zammitt and others on Malta Fever, by which the disease has almost disappeared from certain areas where it was formerly endemic, of Pasteur in relation to hydrophobia, and of a host of workers on yellow fever, among whom one is glad to place my illustrious predecessor. I cannot,

however, pass from this without paying a tribute to at least three men, Carroll, Myers and Dutton, who gave up their lives in this great work. They are, however, only three out of many who have paid the price of the risks which they have run in the interests of humanity—and paid it with their lives.

Of tuberculosis it is hardly necessary to speak. The first great advance was made by the discovery, by Koch, of the causal bacillus, and to-day it is not too much to say that we have it in our power to crush out this fell scourge of humanity. This is not the place to criticise modern policy in relation to this disease. We have accomplished much in the past, and there are signs that in the near future we shall do much more, but to-day we are only touching the fringe of this great problem—and yet, I repeat, we have it in our power to stamp out the disease in a comparatively short space of time. But we, as a nation, have not yet risen to that standard when we shall demand that the first duty of a Government, and not almost the last, is to concern itself with the health of the nation—that it is better to spend money—yea, even to squander money—in an effort to strengthen the physique and improve the well-being of the people than to spend it on what, to say the least, is a luxury—such as palatial buildings, whether these be for government officials, for the vagrants and outcast of our great cities, or even for pauper lunatics.

Our conquests in the past have been, as I have said, very great, and we can look forward to the future with the greatest hope. Let it not be thought, however, that the task before us is a light one. There are problems of the greatest gravity to face—problems which call for real hard work and

balanced judgment. Tuberculosis, as I have said, has not yet been completely conquered—rheumatic fever, which brings with it, so often, such dire consequences, particularly to the young, blasting many a life of brilliant promise and converting the vigorous and the robust into the permanent invalid, is still one of the plagues of our time, though some of us believe that we have already got to real grips with it. Scarlet fever and those two diseases, whooping cough and measles, which, unfortunately, so many regard as of no consequence, but which are amongst the most deadly diseases with which the medical officer has to deal, are still to be conquered. It has been well said that if bubonic plague were to kill as many children in one year as whooping cough, and we might add measles, the whole world would quarantine against our country. A child dead of whooping cough is just as dead as a child dead of plague; and a child whose body is weakened by disease—a result so common after these scourges of the young, is a potential economic loss to the nation. ‘Whooping cough, and we may again add measles,’ says Dr. Rücker, ‘are dangers to be avoided and combated in the interests of humanity and the citizens of to-morrow.’ And yet many a mother has exposed her child to these diseases in the firm belief that she was performing a good act, and in ignorance of the jeopardy in which she placed the life of one whom she loved. It is safe to say that these common diseases not only take a heavy toll of the children of our land directly, but that they are also followed by results which may cripple the child or shorten its life. Even for these diseases we are not without hope that the conquest is at hand, and we are all now anxiously awaiting the confirmation of experimental

work with measles which promises to give us the key to the solution of the causal agent of this disease—the first step in the road to prevention and cure. But it is not only in the sphere of infectious disease that progress has been and is being made. Much has been done, and much is still being done, to ensure the purity of our water supplies and of our foods. The ravages of tuberculosis, owing to a milk supply derived from infected cattle, and the lamentable waste of human life which is constantly taking place, owing to a preventable contamination of our milk and other foods, call loud for redress. The great infant mortality due to summer diarrhoea, which our milk depôts are doing much to minimise, is only too obvious proof that we are still far from the full solution of this great problem, and surely to young men who are just embarking on the at present troubled sea of medicine, there can be no finer prospect than to enlist in the ranks with those who are fighting these great questions, and to share in the victory which must bring with it, not perhaps monetary gain, but at any rate the satisfaction of having done something to alleviate human misery and human suffering.

Ladies and Gentlemen, I do not claim that all the great work of the past for the stamping out of certain diseases and the lessening of the risk of others is due to the bacteriologist. He certainly has given the key which has opened many a closed door, but it is to the pathologist we owe the foundation on which the modern superstructure of bacteriology has been built, and I am firmly convinced that when the bacteriologist and the pathologist become really divorced, both sciences will suffer enormously. A study of the biological characters of bacteria, combined with detailed work on their physiology,

is valuable, but if this is not associated with a thorough knowledge of the pathological processes produced, therapeutic advance is almost impossible, and this should be the aim, at least, of the medical bacteriologist. The clinician, too, has played his part, and if it would be disastrous for the pathologist and the bacteriologist to be divorced, it will be almost equally fatal to progress if these two workers confine themselves to their slides and their culture tubes and do not keep themselves in touch with the man at the bedside. Perhaps most important of all is the association of the bacteriologist with the medical officer of health. To the latter is entrusted, among many other things, the control of infectious disease, and the purity of our food supplies. His knowledge is invaluable to the laboratory worker in this field, and, on the other hand, without the special knowledge which he gains from the bacteriologist, his work would be robbed not only of much of its interest, but of a great part of its value to the community at large.

Thus, my picture of the future is not one of separation, but of union. The time was when physician, pathologist, bacteriologist and medical officer of health were one and the same individual: this plan, no doubt, had its advantages, but the growth of knowledge in each of these spheres of work has been so great that it has become impossible for any one man to carry out with any degree of success such multiple duties. The specialist in each subject has become a necessity, but in this very necessity there lurks a great danger. The experimental pathologist of to-day fully realises that if he is to investigate disease properly he must have some sort of control over the diseased person—hence the appeal

for the establishment of so-called Research Hospitals. The bacteriologist, too, has joined hands with the pathologist in this agitation, for he realises that his work will be done with greater intelligence, and can only be done with success if he has the full facts before him—facts which have been gathered by the clinician on the one hand, and the medical officer of health on the other. This tendency of the bacteriologist to realise that if he has to do his work successfully he must go out of his laboratory into the world, if I may so put it, was well illustrated in a discussion on our water supplies, which took place in this city at the meeting of the British Medical Association this year. In that discussion most eminent authorities on this subject agreed that it was imperative to insist on the necessity of the results of the bacteriological examination of water being considered along with the result of a personal inspection of the locality. So strong, indeed, was this opinion held that the meeting unanimously passed the following Resolution :—

‘That this Conjoint Meeting of the Sections of State Medicine and Bacteriology unanimously desires strongly to urge that no opinion as to the quality of a water for dietetic purposes should be arrived at on bacteriological evidence without a local and topographical inspection of the sources of the supply made by a competent person.’

This only serves to shew the trend of modern bacteriological opinion—the work in the laboratory must be checked and controlled by the work in the field.

This co-operation of workers has been realised both on the Continent of Europe and in America, and the research institutes, which have probably their highest ideal in the Rockefeller Institute in

New York, are so constituted that a great problem can be tackled from all sides. The bacteriologist, the chemical physiologist, the pathologist, the medical officer of health, and the clinician work side by side with the one aim—and that not to establish for himself a place in the roll of fame, but to mutually assist in the solution of the great problem which perhaps one of them has propounded. During the review I have given you, you could hardly fail to be struck by the comparative absence of English names from the list of those who have greatly advanced the science of bacteriology. But the cause is not far to seek. No doubt we have been hindered by legal restrictions which have been drawn up in the interests of humanity, as if sparing a little pain to a guinea-pig were to be more considered than the saving of the life of a man—perhaps some brilliant leader of the nation, or it may be the only support of a widowed mother. But this is not the only cause. Koch and his colleagues working in his laboratory, and Pasteur with his associates working in his institute, account for a large proportion of the workers. Co-operant, and not specially individual, work is the keynote of success. France, Germany, Switzerland, and America, not to mention other countries, have institutes scattered over their land where encouragement is given to anyone capable and willing to undertake research work, and the work is conducted into the right channels by the heads of these institutes: with us I fear there is not that enthusiasm for research. We offer no inducements to men. There is no money for this kind of thing. We are a commercial people, and we like to see an immediate return for our money. There are men, and we in this University have been specially blessed with them, who have invested for

the future and are prepared to wait for their interest. Grateful as I am to two men who have given so much for the endowment of research, it always seems to me rather to our discredit that in the one case we are indebted to America, and in the other to South Africa. We have one great general institute now commemorating the name of Lister, but surely one is hardly fit recompense for the great deeds which have been done through and in the name of Preventive Medicine. There has been an enormous saving of human life and human suffering.

Take these two pictures :—

Mr. J. M. Keating in his *History of the Yellow Fever Epidemic of 1878 in Memphis*, says :

‘ Men, women and children poured out of the city by every possible avenue of escape. In vain the railroad officials plead, in vain they increased the accommodation. The stream of passengers seemed to be endless, and they were mad as they were many. The ordinary courtesies of life were ignored, politeness gave way to selfishness, and the desire for personal safety broke through all social amenities. . . . To the cities of the far North and the far West they fled, too many of them to die on the way like dogs, neglected and shunned, as if cursed of God. At night the city was silent as the grave, by day it seemed desolate as the desert.’ And why all this? Because the pestilence came from the unknown—and no one knew how to check it.

But contrast this, painted by the master hand of one whom we all knew. Sir Rubert Boyce describing the state of things during the Yellow Fever Epidemic in New Orleans in 1905, writes :

‘ When I alighted from the car the first thing that greeted my eyes was a placard, of which tens of thousands had been distributed—bearing the words :

“Wear a smile on your face and a flower in your buttonhole.”

‘That is the spirit which has led to victory. To go at the enemy laughing, and to fight with a jest, means always to overcome with a glorious victory. . . The municipality was organised in wards, the medical profession was organised by flying squadrons; the State was organised by parishes. With a tangible enemy in view, the army of defence could begin to fight rationally and scientifically. Every individual citizen of every city and town, but especially of New Orleans, had taken to himself new heart of grace, because he knew what he had to fight.’

Yes, terror did not take possession, because through the great work of the Yellow Fever Commission—which paid its great price in the lives of Lazear, Carroll and, later, Walter Reed—men knew the enemy and how to fight it.

These pictures represent the changes in one sphere, but the history of others might be written in a similar strain. The financial gain to us as a nation cannot be counted in thousands or in tens of thousands, but what is this to the saving of life and the reduction of human suffering—and yet—I repeat it—we have still only one properly-endowed institute of Preventive Medicine in the whole of the British Isles. Surely we have not as a nation paid the debt we owe to the Medical Sciences.

